

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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No. 5.

PROFESSOR ADAMS, F.R.S., President, in the Chair.

George Russell Rogerson, Esq., Waterloo, near Liverpool, and
Richard Cross, Esq., Forest Hill,

were balloted for and duly elected Fellows of the Society.

The following Resolution was passed at the last meeting of the Council:—

Resolved:—That all books belonging to the Society, now in the hands of Fellows, be returned to the Assistant-Secretary at Somerset House, on or before the 1st of May next, after which date, and until further notice, no book can be taken out of the Library without the express permission of the Library Committee.

*Notes to accompany Chromolithographs from drawings of the planet
Jupiter, made with the Six-foot Reflector at Parsonstown,
in the years 1872 and 1873.*

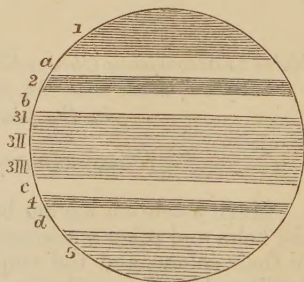
(Communicated by the Earl of Rosse, F.R.S.)

Much attention having been given of late to the appearance of the belts and other markings on *Jupiter*, more especially in regard to their colour, and the employment of the pencil having been facilitated by the application of a clock-movement to our Six-foot Reflector, we were induced last season to try how far the more delicate markings and tints on the planet's surface, which might

be beyond the reach of smaller instruments could be brought out by a larger aperture. The disadvantage which the limited range* of that instrument in right ascension entailed in the delineation and continuous study of the physical features of a rapidly revolving planet, together with the circumstance that more advantage is derived from increase of aperture in the examination of *fainter* objects, had been the cause of most, possibly too large a proportion, of our attention having been directed from the first to the Nebulæ. In the short space of time available, it was found impossible to do more than take hasty sketches at the telescope accompanied by notes of the colours and general appearance of the belts. From these sketches water-colour drawings, of which the accompanying lithographs are faithful copies, were made, generally on the following morning. To render each drawing as independent as possible of any preconceived bias, the colours were invariably mixed afresh, and in no case was a drawing compared with any of those already made until it was completely finished. The drawings have been executed by Dr. Copeland, usually observing alone. The whole of the notes are given as they were written down at the telescope. Although many of these may perhaps appear to be unnecessary, as only expressing in words what is given in the drawings, a careful comparison will show that there are many points of divergence between drawing and description, so that the one may be regarded as to some extent supplementing the other. There are also many nuances of colour and peculiarities of form represented so faintly that they might readily be overlooked or considered as accidental unless special attention was drawn to them. From the fact already mentioned, that each sketch was coloured without any reference to the others, it is scarcely to be wondered at that colours which are named alike in the notes, show widely different tints in the drawings.

To make the notes intelligible without the necessity of inserting letters or other marks in the drawings, the following nomenclature was adopted. The dark zones are marked 1, 2, 3, 4 and 5, as shown in the figure, and the bright ones *a*, *b*, *c* and *d*. In the case of the equatoreal zone, 3, the Roman numerals I, II and III, are appended to express subdivisions; in all cases the smaller numbers and earlier letters refer to the southern or upper part of the disk.

Unfortunately the drawings have not been arranged, as we had intended, in regular order of the rotation of *Jupiter*, owing to our having been misled by an error in



* At most, 40 minutes for an object near the Equator.

Sir John Herschel's *Outlines* (Art. 512, p. 340, 8th edition), where the period of $9^h 55^m 50^s$ mean time is marked sidereal time.

The error was only detected, after the lithographs had been struck off, on reading Dr. Schmidt's article in No. 1973 of the *Astronomische Nachrichten* and then referring to No. 1541 of that journal. The correct values of L , the Jovicentric longitude of the true centre of the planet's disk, are given below. After the correction the drawings gain much in interest from the possibility of tracing a brick-red marking through many revolutions. The zero of longitude or first meridian of *Jupiter* is that which *Jupiter* would have presented to the Earth at noon on December 31, 1872, had his geocentric longitude been the same as at the moment of the opposition of 1873. The time at which the light left *Jupiter* is also given for each observation.

Beer and Mädler's period of rotation, $9^h 55^m 26^s.5324$ mean time*, has been used in calculating the values of L .

Arrangement of Sketches in order of the Rotation of Jupiter.

No.	Date (Greenwich mean time).		L_0	Time when light left <i>Jupiter</i> .	
9	1873	Apr. 1, 9 49	3°0	9	10°0
14	"	Apr. 11, 8 58	38.2	8	18.3
10	"	Mar. 13, 10 59	63.4	10	21.9
6	"	Feb. 7, 13 34	72.5	12	57.6
5	"	Jan. 26, 14 27	95.4	13	50.0
17	"	Mar. 11, 11 13	130.3	10	35.7
16	"	Feb. 27, 12 5	153.7	11	28.8
12†	"	Feb. 10, 13 21	156.7	12	44.3
8	"	Jan. 24, 14 36	159.1	13	58.5
19	"	Feb. 20, 12 36	197.3	11	59.9
15	"	Feb. 3, 13 52	200.1	13	15.1
7	1872	Dec. 31, 16 15	201.4	15	36.1
11	1873	Jan. 17, 15 11	205.1	14	33.9
18	"	Feb. 8, 13 30	220.6	12	53.2
13	"	Jan. 22, 14 44	222.8	14	7.1
3	"	Apr. 10, 9 12	256.1	8	32.4
2	"	Mar. 7, 11 33	259.9	10	56.3
1	"	Feb. 6, 13 38	284.4	13	2.0
4	"	Mar. 22, 10 21	316.0	9	42.7

The ratio—

Polar semidiameter = Eq. semidiameter $\times 0.927$ —
was adopted from the *Nautical Almanac*, for the form of the

* See *Beiträge zur physischen Kenntniss der himmlischen Körper im Sonnensysteme*, by Wilhelm Beer and Dr. J. H. Mädler. Weimar 1841.

† No. 12, the lithograph is wrongly dated January 22.

disk; the equatoreal diameter of the drawings is 50 millimètres.

The magnifying power most used was 414; but in disturbed states of the atmosphere a lower power of 281 or even 120 often gave better definition, with these however only 4 ft. 8 in. and 2 ft. respectively of the large mirror came into use.* On rare occasions power 650 could be used with advantage.† As all markings on the surface of the planet when situated near the edge of the disk have always been noted by us as being excessively faint and dim, little attention was paid to any traces of phase, and no attempt has been made to delineate it.

Notes on the Colours and general Appearance of the Belts.

Opposition of 1872.

January 15, 1872, 12^h 55^m Greenwich mean time. 1 grey; a yellow patch at the northern end of the dark marking in 2; 3 yellow, there is also a narrow streak of yellow on the northern edge of 4; 5 bluish. The planet was at times wonderfully well defined with power 414.

February 7, 1872, 11^h 12^m ± G.M.T. 1 of a very uniform grey tint; white patches on the margin of the yellow equatoreal belt; 4 of a purplish neutral tint. Sketch very hurried.

Opposition of 1873.

No. 7; December 31, 1872, 16^h 15^m G.M.T. $L = 201^{\circ}4$.

In 3 I and 3 III are dark clouds; there is a bright diagonal rift in 3 III, and a bright central area in 3 II. The tints noted are: 1 greyish; *a* white; 2 greyish; *b* very white; 3 yellowish red; 4 the darkest part of the disk, and lastly 5 of a pale ashy blue. The equatoreal belt is not nearly so red as it was in the spring of 1872. The central area of 3 II is about the brightest part of the disk. 5 is darker than 1. There is a streak of yellow along the northern edge of 4.

No. 11; January 17, 1873, 15^h 11^m G.M.T. $L = 205^{\circ}1$.

b is very white and breaks in upon the equatoreal belt near the following (right) side, forming a white gulf, following which is a yellowish brown patch; *c* is white. The equatoreal belt is not nearly so intensely coloured as it was last year, the centre being almost colourless—grey and white—but bounded north and south

* It is assumed that the aperture of the pupil was $\frac{1}{2}$ inch, probably too large an estimate in the case of so bright an object as *Jupiter*. On no occasion were diaphragms used to reduce the aperture of the telescope, nor indeed is any provision made for their application.

† Powers 650, 281, and 414 are achromatic positive eye-pieces; the two former by Mr. H. Grubb, the latter by the late Mr. Cooke; their performance is very good in all parts of the field.

by more highly coloured regions. Sat. I as seen to emerge from behind the planet was of a deep yellow colour (shown in the drawing) and much fainter than the planet. There is a marked projection in the middle of the south side of the equatorial belt. Definition good for a few very short intervals, otherwise rather poor.

No. 13; January 22, 1873, 14^h 44^m G.M.T. $L = 222^{\circ}8$.

1 yellowish; a reddish or reddish-yellow patch on the following side of *b*; a number of large and small, very white spots in 3 II; the northern polar zone (5) of a very uniform slate-grey.

No. 8; January 24, 1873, 14^h 36^m G.M.T. $L = 159^{\circ}1$.

The sky very hazy, with definition in rare moments only. Nothing more than a rough and hurried sketch taken. The tints are, 1 yellowish; *a* white; *b* very white; *c* very white; 4 purplish and the darkest part of the disk, and lastly 5 of a bluish grey.

No. 5; January 26, 1873, 14^h 27^m G.M.T. $L = 95^{\circ}4$.

The large, black spot is the shadow of Satellite III. Definition mostly poor, but still the large oblique white patches on the equatorial belt are occasionally well seen. The darkest patch is a little south of III Sh. Belt 2 fades away on the following side; zone 5 is as usual bluish grey.

No. 15; February 3, 1873, 13^h 52^m G.M.T. $L = 200^{\circ}1$.

The preceding and following parts of 3 I are reddish, the former inclining to brown and the latter to a very red copper colour: large white clouds in 3 II, small ones and a rather long white streak in 3 III; northern pole bluish grey. The northern edge of 3 is very ragged. Definition occasionally good, otherwise very poor; the long, narrow, grey cloud seen very well repeatedly, as also the bend and shading of belt 2, but the equatorial spots are not so well defined as on other occasions. The Sat. III noted as being $1\frac{1}{2}$ time the diameter of Sat. II.

No. 1; February 6, 1873, 13^h 38^m G.M.T. $L = 284^{\circ}4$.

1 yellowish with a whitish patch on the preceding side; *b* very white on the following, reddish yellow on the preceding side: *c* very white; 4 neutral tint—darkest part of the disk—bounded by a narrow, yellowish line on the north; 5 blue-grey. Powers 120, 281, and 414. The following features were noted as being remarkable: 1st, the *right-handed* inclination of the cloud crossing the Equator; 2nd, the immense reddish yellow area extending far to the south on the preceding side, and 3rd, the fact of belt 2 being, so to say, *forced up* into contact with 1. There is only the slightest separation (not shown in the lithograph) of the preceding part of the dark boundary of 1 from the rest, in the other

part 2 is completely connected with 1, which is decidedly yellow in contrast with the blue of 5. Belt 4 is much darker than usual, with the neutral tint of last year and the band of yellow on the north. The patches on 3 are of a pale brownish yellow.

No. 6; February 7, 1873, $13^h 34^m$ G.M.T. $L = 72^\circ 5$.

Definition occasionally good; power 281. *b* very white; 3 yellowish with white patches; 5 greyish. The belt 2 is much darker than usual, being nearly the darkest part of the disk; its following end, which is bent towards the north and terminates before reaching the limb, is separated from the equatoreal belt by a very white interval that seems to be composed of rounded cloudy masses. The south edge of 3 is formed of two fine parallel lines of dark cloud, the other markings being very irregular with large 'port-holes.'

No. 18; February 8, 1873, $13^h 30^m$ G.M.T. $L = 220^\circ 6$.

A reddish yellow patch on the following side of *b*; the dark spot in 3 II is of a full blue colour, while the white spots are intensely white and bright. The middle of *b* is composed of white clouds; *d* very white; *c* also white, but not quite so much so as *d*; 5 bluish grey. 3 III exhibits a long line of very small white clouds. The driving clock very troublesome (owing to recent alterations), many clouds are also passing over, but between them the definition is remarkably good, and all the markings drawn are well seen. There is no mistake about the blue colour of the spot in 3 II, as it was repeatedly examined in all parts of the field of view. When the clouds pass over the planet it is at once evident that the centre of the disk is much brighter than the margin.

No. 12; February 10,* 1873, $13^h 21^m$ G.M.T. $L = 156^\circ 7$.

b very white; 3 I reddish brown, as also 3 III; large white patches on 3 II, the brightest of which, near the preceding side of the disk, is the brightest marking on the planet; 5 slate colour. Seen very well for a few seconds between clouds; the clouds never completely obscured the planet, so that as their increasing density obscured one part after another, the order of brilliancy of the various regions of the disk could be readily estimated. Tested in this way the north polar zone (5) would seem to be about the darkest part of the disk, for when the planet is nearly obscured that zone is much darker than any other region. The belt 4 disappears rather on account of its narrowness than its blackness, so that the depth of its shading cannot be compared with that of the broader belts by this method. The central part of the equatoreal belt is on the whole very bright, but the spot already mentioned, on the preceding

* The lithograph is wrongly dated January 22.

side, is considerably the brightest part, remaining visible as a bright oval disk when the rest of the planet is nearly obscured.

No. 19; February 20, 1873, $12^h 36^m$ G.M.T. $L = 197^\circ 3$.

b is composed of white clouds; there are also a number of larger and smaller white clouds on 3 II and 3 III; 3 I is reddish, and *d* very white; while 5 is noted as dingy; 4 is as usual the darkest part of the disk. Almost all the red colour is now confined to the southern part of the equatoreal belt, the rest of which is much broken up; the dark marking well seen—not blue. A small dark object passed rapidly (almost instantaneously) across the planet from north—preceding to south—following (Query, a meteorite?).

No. 16; February 27, 1873, $12^h 5^m$ G.M.T. $L = 153^\circ 7$.

The belt 2 is ragged on both edges, and has a number of very small white clouds on it; 3 I is reddish, with exceedingly small white clouds; 3 II and 3 III of a very faint yellow, with very white clouds, the black spot is the shadow of Sat. II; 4 is purplish, with a yellowish streak on the northern edge; 5 is bluish.

The dark lines on the equatoreal belt are fully darker than 4. The south polar zone, which is about half the width of the northern one, is at times suspected to be rough on the edge. Definition only very rarely good. Powers 281 and 414.

No. 2; March 7, 1873, $11^h 33^m$ G.M.T. $L = 259^\circ 9$.

a and *b* are very white, the following part of *b* is reddish; 3 II and 3 III show white patches.

No. 17; March 11, 1873, $11^h 13^m$ G.M.T. $L = 130^\circ 3$.

1 very faint; 2 is composed of very white and very irregular clouds; 3 I is reddish and 3 III yellowish; on the preceding side of 3 is a large bluish grey marking; 5 greyish blue; 4 the darkest part of the disk. The north polar zone was not seen bluish at first, but on closer examination it is certainly not yellow. Definition rarely good; all the markings, except the two dark streaks, are very faint.

No. 10; March 13, 1873, $10^h 59^m$ G.M.T. $L = 63^\circ 4$.

1 is very faint; in 2 there are two patches darker than the rest; *b* and the patches in 3 are very white, the following patch in 3 II being the brightest; 3 I is yellowish; in 3 there are two bluish spots, of which the following one is the darker; 4 is again the darkest part of the disk, and of a neutral tint. Definition rather good.

No. 4; March 22, 1873, 10^h 21^m G.M.T. $L = 316^{\circ}0$.

a very white; 1 yellowish; there is a reddish spot on the preceding side of 2 and *b*; an excessively white patch in 3 II, also a dark blue spot in the same zone; 4 is as usual neutral tint; *d* very white, and 5 bluish. Definition pretty good, with powers 281 and 414.

No. 9; April 1, 1873, 9^h 49^m G.M.T. $L = 3^{\circ}0$.

Belt 2 is plainly interrupted; there is a reddish streak in 3 I, as also a dark streak certainly visible in *c*; belt 4 is very narrow.

No. 3; April 10, 1873, 9^h 12^m G.M.T. $L = 256^{\circ}1$.

b is very white, with a reddish patch on the following side; the patches in 3 I are very white; 4 is the darkest part of the disk—neutral tint; there is a blue patch in 3 II, and one of a darker blue tinge in 3 III; 5 is bluish grey. Definition very good, with powers 281 and 414. The inclined streak with its bend is very well shown. The red patch in *b* seems to break off abruptly on the following side. There is a narrow band of red on the northern edge of 3. The irregularities in 2 are very well seen; 4 is very narrow, and thought to be bordered with red. The inclined belt and the southern edge of 3 are composed of parallel dark streaks, as shown in a drawing previously made (compare No. 13, January 22).

No. 14; April 11, 1873, 8^h 58^m G.M.T. $L = 38^{\circ}2$.

2 of a purplish or dark neutral tint; *a*, *b*, 3 I, parts of 3 II and 3 III, as also *c* and *d*, are white; the dark cloud in 3 III is bluish, 4 is purplish, and 5 bluish grey. Definition at times almost perfect, with powers 281, 414, and 650.

Before attempting to deduce any results from the foregoing observations, it may be well to say a few words about two published sets of drawings of *Jupiter* made contemporaneously with our own. The first of these is contained in "*Observations de Jupiter et de Mars faites à Louvain pendant l'opposition de ces planètes en 1873; par M. F. Terby, docteur en sciences.—Extrait des Bulletins de l'Académie royale de Belgique, 2me série, tome XXXVI., No. 11, 1873.*" There are 14 drawings of *Jupiter*, made with a telescope of 9 centimètres ($3\frac{1}{2}$ in.) aperture, and powers 120, 186, and 240. Assuming the dates given to be Louvain mean time, we have reduced them to Greenwich mean time, using 18^m·8 for the difference of meridians, and have calculated the values of *L* to facilitate comparison.

M. Terby's No.		Greenwich M.T.	L°	Time when light left <i>Jupiter</i> .	
				h	m
T 1	1873, Jan. 23,	10 6·2	205·4	9	29·1
„ 2	„ Feb. 28,	8 3·7	158·3	7	27·1
„ 3	„ Mar. 8,	6 23·7	223·4	5	46·7

M. Terby's No.		Greenwich	M.T.		L. °	Time when light left <i>Jupiter</i> .	
			h	m		h	m
T 4	1873,	Mar. 8,	8	3·7	283·9	7	26·7
" 5	"	" 23,	6	21·2	321·8	5	43·1
" 6	"	" 24,	8	6·2	175·9	7	28·0
" 7	"	" 25,	6	26·2	266·0	5	47·9
" 8	"	" 25,	7	58·7	322·0	7	20·4
" 9	"	" 26,	7	48·7	106·6	7	10·3
" 10	"	" 27,	7	56·2	261·8	7	17·7
" 11	"	" 31,	7	38·7	133·7	6	59·9
" 12	"	Apr. 1,	6	16·2	234·4	5	37·3

Nos. 13 and 14 we have omitted, as they only profess to represent the transit of the 4th Satellite.

The other set of drawings are those published by Mr. Knobel, in *Monthly Notices*, vol. xxxiii. p. 474; these are three in number, for which we find the following quantities:

Mr. Knobel's No.		Greenwich	M.T.		L. °	Time when light left <i>Jupiter</i> .	
			h	m		h	m
K 1	1873,	Mar. 26,	7	50	107·4	7	11·6
" 2	"	Apr. 20,	7	30	259·7	6	48·9
" 3	"	May. 11,	8	15	207·3	7	31·2

The drawings were made with a silver-on-glass reflector of $8\frac{1}{2}$ -in. aperture, and a power of 208.

Of all the features presented to our view by *Jupiter* during the opposition of 1873, probably the most remarkable is the great break in the southern side of the equatoreal belt in $L=260^\circ$. It is shown in Nos. 2, 3, 7, 11, 13, 15, and 18, it occurs most unmistakeably in K 2 and 3, and is also indicated by the markings in T 3 and 4, and perhaps to some extent also in T 7, 10, and 12. On December 31, 1872 (No. 7), it would seem that the break had either not fully occurred, or that it was very imperfectly seen, being 35° following the centre of *Jupiter's* disk; on January 17, 1873, it was fairly seen. On January 22 and April 10 (Nos. 13 and 3), it was seen to great advantage under most favourable circumstances; on both these days the preceding part of the southern edge of the equatoreal belt consisted of two parallel lines of dark objects (clouds?) separated by a clearer interval, but with this remarkable difference, that whereas on the former occasion both lines were curved towards the Equator at their following ends, on the latter, only the southern one was so curved, thus causing it to coalesce with, and possibly to intersect the other. This union appears to have been very permanent, for it was well seen by Mr. Knobel on April 20 and May 11. Following and filling up this break is a brick-red area, that was seen most fully on February 6 (No. 1), the chromolithograph for which day is, however, too faintly tinted. The red region may extend

some 30° in longitude, reaching from $L 250^\circ$ to $L 280^\circ$: its following end is just seen in sketch 4.

Since the break in the edge of the equatoreal belt is indicated by the following end of the long, curved marking in our earliest drawing, and is fully shown by the intersection of the dark belts in Mr. Knobel's latest drawing, and was depicted with more or less certainty on eight other occasions, there seemed to be a chance of determining the period of rotation from it with some considerable accuracy. Taking therefore the Parsonstown Sketches 2, 3, 7, 11, 13, 15 and 18, also K 2, K 3, and T 3; further, using the times when the light left *Jupiter* on each occasion (as a means of correcting for the varying distance of the planet), also allowing *in time* for the distance of the point selected for observation from the centre of the planet's apparent disk, and lastly making a still further small time-allowance for phase, the following summary was arrived at:—

Corrected time when the point was at the true centre of the planet's disk.			Rotations elapsed.
1873.	G.M.T.		
d	h	m	
0	16	34.7	0
17	15	23.8	41
22	15	5.0	53
34	14	44.9	82
39	14	23.0	94
66	11	0.4	159
T 3	67	5 43.1	161
	100	8 45.8	241
K 2	110	7 25.6	265
K 3	131	8 41.7	316

Putting then t = the error, in minutes of time, of the first observation, and ΔR = error of Mädler's period, and combining the first observation with each of the others in succession, we have the following equations of condition:—

$$\begin{aligned}
 0 &= -4^m 40 + t + 41 \Delta R \\
 0 &= +31.9 + t + 53 \Delta R \\
 0 &= +23.9 + t + 82 \Delta R \\
 0 &= +56.7 + t + 94 \Delta R \\
 0 &= +30.4 + t + 159 \Delta R \\
 0 &= -37.8 + t + 161 \Delta R \\
 0 &= +29.5 + t + 241 \Delta R \\
 0 &= +58.7 + t + 265 \Delta R \\
 0 &= +7.3 + t + 316 \Delta R
 \end{aligned}$$

The complete solution of these equations gives $t = -18^m 44$

+ $14^{\text{m}}.15$; $\Delta R = -0^{\text{m}}.02172 \pm 0^{\text{m}}.07758$ and the probable error, r , of a single observation = $\pm 21^{\text{m}}.65$. Substituting the values of t and ΔR in the equations, we have the outstanding errors shown in column α of the following table:—

α m	β m	γ m
-23.3	-30.4	-27.2
+12.3	+ 5.2	+ 7.8
+ 3.7	- 3.5	- 2.1
+36.2	+29.0	+29.8
+ 8.5	+ 1.0	- 0.9
-59.7		
+ 5.8	- 1.9	- 7.4
+34.5	+26.7	
-18.0	-26.0	

The large outstanding error for the Louvain observation would seem almost certainly to arise from the time of M. Terby's No. 3 being given 1^{h} too early; the correctness of this surmise is rendered almost certain by the fact that the preceding curved marking in his No. 4, made on the same evening, agrees almost exactly with our selected point, and can only be reconciled with T 3 by assuming an error in the time of the latter to the extent mentioned. Under these circumstances, the Louvain observation was omitted, and the remaining 8 equations being solved again, we obtained

$$t = -25^{\text{m}}.35 \pm 10^{\text{m}}.31, \Delta R = -0^{\text{m}}.02527 \pm 0^{\text{m}}.05578, \\ r = \pm 15^{\text{m}}.57.$$

The outstanding errors are given above in column β .

As a further experiment, the Parsonstown observations were used alone; they gave

$$t = -20^{\text{m}}.43 \pm 10^{\text{m}}.97, \Delta R = -0^{\text{m}}.06836 \pm 0^{\text{m}}.08553, \\ r = \pm 14^{\text{m}}.12$$

with the outstanding errors given in column γ .

Figures 10 (March 13) and 14 (April 11) present two features in common, these are the markings in the north part of the equatoreal zone. Taking the centre of the preceding marking, and allowing for aberration and phase, we have the times when it was at the centre of the disk, reckoning from January 0, 1873, $72^{\text{d}} 9^{\text{h}} 44^{\text{m}}.4$ and $101^{\text{d}} 7^{\text{h}} 58^{\text{m}}.8$, 70 rotations having intervened. Hence the period is $9^{\text{h}} 55^{\text{m}} 3^{\text{s}}.8$.

In the same way the preceding end of the following spot occupied the true centre of the planet's disk at $72^{\text{d}} 10^{\text{h}} 29^{\text{m}}.1$ and $101^{\text{d}} 8^{\text{h}} 33^{\text{m}}.9$, giving a period of rotation = $9^{\text{h}} 54^{\text{m}} 55^{\text{s}}.4$. Taking Schmidt's velocity of rotation of the equator of *Jupiter** at 40211

* *Astronomische Nachrichten*, No. 1973.

Paris feet per second, which agrees exactly with Mädler's period, we have for the proper motions of the spots, + 25.6 Paris feet and + 35.1 ft. per second (or 17 and 23 statute miles per hour), both with a probable error of + 13.6 ft. The probable error is obtained from the result already arrived at, that the interval of time between two of our drawings, as deduced from the original sketches, has a probable error of $\pm 14^m.12$. It is well known that results of far greater accuracy may be obtained by noting the exact time when any particular marking passes the centre of the disk, but the limited range of our instrument almost entirely precludes the use of this method.

In fig. 19 there is a dark marking in the equatoreal belt that may be recognised in M. Terby's No. 6. A complete reduction of these observations gives $31^d 19^h 8^m.2 = 77$ rotations, for the interval elapsed: whence it follows that the spot had a proper motion of + 53.5 Paris ft. + 12.7 ft. per second, or + 36.9 miles per hour.

Schmidt, in *Ast. Nach.* No. 1973, gives a number of cases of proper motion of spots on *Jupiter*, ranging from 10 ft. to 297 ft. per second.

The aspect of the planet given in fig. 16, February 27, will be found represented with as close accordance as can be expected, considering the difference of the instruments used, in M. Terby's No. 2, which was taken two rotations later. The curved marking called *a* by M. Terby can be immediately identified in the Birr drawing; but it would seem to be hardly possible that it is identical with either of those marked with the same letter in his Nos. 9 and 11, but rather that all three markings are entirely distinct. The *a* of M. Terby's Nos. 2 and 11 are *both* shown in our fig. 16.

The blue spot seen March 22 (fig. 4) does not seem to have been very permanent, unless indeed it is shown near the following limb of fig. 1. It was figured by M. Terby once on March 23, and twice on March 25. Unfortunately this side of the planet has been little observed by us, and M. Terby's drawings were made too close to our's in point of time to allow of any deductions being made.

It may not be uninteresting to remark, that Mr. Knobel's No. 1 and M. Terby's No. 9 were taken at almost the same moment.

The nights, January 22, April 10 and 11, 1873 (Nos. 13, 3 and 14) far exceeded most of the others in point of definition; the last night in particular affording almost uninterrupted views of the most delicate markings. It will be readily understood that the definition with a six-foot aperture is, even on the best nights, more or less fitful; often for minutes together all the finer details of a brilliant object like *Jupiter* are mixed up in inextricable confusion, and it is only at more or less widely separated intervals that the confused image suddenly appears to freeze or crystallise into one of great sharpness. It is only in

such moments that details like the small white clouds in I 3 of No. 16 (not particularly well seen in the chromolithograph), or the small dark streaks shown in Nos. 3, 13 and 14 can be seen at all. It seems probable that the latter markings are closely related to those described by Secchi, in the *Memorie del nuovo Osservatorio del Collegio Romano*, 1852-1855, p. 114, with this difference, that what he describes as "a great multitude of bright lines on the obscure part beneath the principal band,"* we have always seen as a number of fine dark lines on a bright ground.

Similar appearances are mentioned by Mr. Webb† as having been described by Schwabe and Jacob, but nothing of the kind is shown in the well-known magnificent drawings by Mr. De La Rue and Professor Piazzi Smyth.

The great loss of colour sustained by the equatoreal belt within the last two years has been a subject of general remark: its extent will be best seen by comparing the two rough drawings of the opposition of 1872 with the others. The redness of that belt in the autumn of 1870 was such that, according to a naked-eye observation by one of us in September of that year, the general colour of the planet's light was affected by it. This observation was made without a previous knowledge of the fact that *Jupiter's* belts were at all redder than usual.

It is very remarkable that while the southern and equatoreal regions of *Jupiter* during the opposition of 1873 were subject to such great changes, the northern regions, and especially the dark belt 4, remained so long unaltered. There is, however, now an end to this state of things, for, on February 22, 1874, 14^h 37^m G.M.T. the planet was seen without a trace of the northern temperate belt: the equatoreal belt was fawn coloured as in fig. 7.

* "... una gran moltitudine di linee chiare nella parte oscura sotto la fascia principale."

† *Celestial Objects for Common Telescopes*, 2nd edition, p. 127.

On Two Ancient Conjunctions of Mars and Jupiter.

By the Rev. Samuel J. Johnson.

As a supplement to the ancient Conjunctions referred to in the *Monthly Notices* for January, two important ones may be found in Street's *Astronomia Carolina* (1661), which I have not found mentioned by any more recent authors, and they, perhaps, merit further examination. Street gives the results obtained from the tables of his day. He states:

"Anno Christi 498. May the 1st day, near 7^h reduced to London, ♄ was seen so conjoined with ♃, that there was no

interval between them. The true longitude of $\odot$ was $0^s 29^m 20^s 6''$. The mean anomaly of Υ $11^s 17^m 42^s 32''$, his geocentrick place $4^s 18^m 31^s 34''$, with lat. north $1^m 25^s 30''$. The mean anomaly of δ $2^s 3^m 7^s 47''$, his geocentrick place $4^s 18^m 32^s 28''$, with lat. north, $1^m 16^s 22''$. The difference of longitude is $0^m 54''$, of lat. $9^s 8''$, at which small distance to the bare eye they might well seem to have no interval or space between them.

"Anno Christi 1170. September the 13th, at midnight, two of the planets were so conjoined that it appeared as if they had been one and the same star, but they were presently separated. *Gervasii Chronicon.*

"These two planets were Υ and δ , being so near together that they seemed as one star, but to some eyes a little distinguished.

"The sidereal longitude of the Sun was by our tables $5^s 5^m 26^s 31''$. The mean anomaly of Υ $7^s 23^m 51^s 50''$, his geocentric place $1^s 19^m 16^s 3''$, with lat. south $42^s 44''$. The mean anomaly of δ $7^s 27^m 13^s 49''$, his geocentric place, $1^s 19^m 8^s 55''$, and lat. south $39^s 1''$. The difference of longitude is $7^s 8'$, of lat. $3^s 43''$, and hence the distance of their centres $8''$."

Upton Helions Rectory, Crediton,
1874, March 9.

On the relative Magnitudes of the Fifth and Sixth Stars in the Trapezium of Orion. By Thomas Barneby, Esq.

Having noticed the discussion at the Meeting of the Society held on the 9th of January last, concerning the relative sizes of the 5th and 6th Stars in the Trapezium of Orion, and having felt an interest in the appearance of those Stars, ever since reading in an early edition of Herschel's *Outlines of Astronomy*, that to perceive *both* was one of the severest tests which could be applied to a telescope, I venture to think the scrutiny I have made of them, with my 9-inch object-glass by Cooke, which I have been told is the best he ever made, may be acceptable to the Society.

On first turning this telescope on the Trapezium some years ago, I saw the 5th star distinctly, but I could not detect the 6th for a considerable time, which caused me some disappointment; but when I first perceived it, I found it so easy that I almost fancied I must have been looking in the wrong place for it before. I could then see it easily with a micrometer-eyepiece, which had evident marks of use by its former owner, the late Captain Jacob. I afterwards from time to time saw the 5th and 6th stars without difficulty, for a considerable period, but I always considered the 5th as by far the more easily seen.

For the last two or three seasons however (before the present),

I have often looked in vain for the 6th star, and I began to think the object-glass was out of adjustment, or my eyesight impaired.

Since the constellation has been near the meridian of my Observatory, at a convenient time of night in the present season, I have occasionally examined the Trapezium carefully, and the following is the result :—

I could not detect the 6th star until the 21st of January last.

On that night I saw the 5th and 6th stars distinctly, but the 5th appeared much the larger and brighter of the two.

On the 4th of February last, I saw both stars again, when decidedly the 6th was the more conspicuous, and apparently much the larger.

On the 8th of February last, I could see the 5th distinctly, but the 6th was invisible to me, and also to my assistant, who has a keen and younger eye.

Now I particularly noticed that the 4th of February, when the 6th star appeared the larger, was the best telescopic night; and it appears to me probable that the 6th is really the larger star, but that partly from its blue colour, and partly from its greater propinquity to its primary, and the irradiation of the primary, it is not to be seen so well as the 5th, except on a superior telescopic night.

Is it possible that the variability in the appearance of the 5th and 6th stars, or either of them, can be occasioned by any *motion of translation in space*, such as that indicated by Mr. Huggins in the neighbouring stars *Rigel* and *Sirius*, and other stars, as well as in *Nebulæ*?

I judged partly of the night of the 4th of February last by the close companion of ζ *Orionis*, which I have found by experience is not to be seen with a clear disk, detached from its primary except on a good night, and on the 4th of February last it was.

There is a great difference in the apparent disks of stars: for instance, the principal star in σ *Orionis* always appears to me as if a flame were creeping about it, and I would direct attention in this respect to λ *Orionis*, and the pointed little reddish bright star, one of the four which attend it, and to the quiet and very beautiful appearance of ρ 1 and 2 *Orionis* which resemble in miniature ‘pulcherrima’ ϵ *Bootis*, all beautiful objects. I found also that, on the night of the 4th of February last, my telescope revealed the neighbourhood of λ *Orionis* bespangled with very minute bright stars, like a loose cluster.

I viewed these objects again on the 7th of March instant, when I saw the 6th star as well as the 5th very distinctly, and I considered the 6th the larger.

The night was again telescopically good, although not so good as on the 4th of February last.

I have never seen the additional stars in the Trapezium.

On each of the nights above mentioned the nebulous appearance

in Orion was very distinctly seen, with its well-known streamers, some of which remind me of the wreaths of smoke which issue from under an extinguisher when putting out the light of a candle.

*Morton House Observatory, Worcester,
1874, March 11.*

Remarks on the Obituary of the late Rev. Professor Temple Chevallier, B.D., Astronomische Nachrichten, 1968, and Monthly Notices, vol. xxxiv. No. 4, pp. 138, 139. By R. C. Carrington, Esq., F.R.S.

In both publications there appears the same paragraph, nearly word for word, although the first is signed John J. Plummer and the last R. J. K. which I take to be R. J. Knight.* I listened with much attention to the reading of the obituary by Mr. Dunkin, and should have risen to contradict the paragraph on the spot if I had heard it, but no, it was not read, and there was not anything in the report to which I could take exception. The paragraph is: "He was the first to institute in England the regular, continuous observation of the Solar Spots, which has since led to important results. The methods he employed in these observations were afterwards adopted by Mr. Carrington (at one time Observer at Durham), who has made a similar series of observations with marked success; and astronomers may perhaps feel disposed to regret that Mr. Chevallier's talents were too much occupied by clerical and professorial work to admit of the full development of his powers in the field of original research."

Now, I hold that Harriot was the "first to institute in England the regular, continuous observations of the Solar Spots," and he observed them from December 8, 1610, to January 18, 1613, as I know from having had his observations in my hands to copy at Petworth House, on September 12, 1857.

Secondly, I have to deny altogether the assertion, twice repeated, that "the methods employed by Professor Chevallier were those afterwards adopted by Mr. Carrington," for I left Durham finally on April 8, 1852, and it is recorded by me in my astrono-

* This may probably be explained on the supposition that Mr. Plummer forwarded to the writer of the obituary notice some notes relating to Professor Chevallier's astronomical work at Durham, and that these notes were also incorporated in the communication sent by Mr. Plummer to the *Astronomische Nachrichten*. We may take this opportunity of remarking that at the January meeting, 1849, Professor Chevallier exhibited a volume containing diagrams and observations, the result of "the regular, continuous observation of the Solar Spots," on which he had been employed some time, and that he expressed his intention of presenting the volume to the Society after he had completed the series of observations. In conformity with this intention, Professor Chevallier presented, on July 2, 1851, his valuable series of observations, bound in two volumes, both of which are now in the Society's Library.—[EDITOR.]

mical journal that, on October 11, 1853, the idea first occurred to me "whilst out walking at Redhill," of taking advantage of the Sun's circularity in the manner which I afterwards adopted, and explained in the *Monthly Notices*, vol. xiv. p. 153, and vol. xv. p. 174. I quote the following from my published work, Preface, p. 2: "To carry out this plan, it was in the first place necessary to devise a new and more commodious method of observation than any hitherto adopted, and to lay altogether new foundations of method in recording, reducing, comparing, and discussing, for I unhesitatingly say that no observer would for any length of time have followed out any of the modes of observation previously practised."

Note on a Paper by Mr. Stone, "On the Rejection of Discordant Observations." By J. W. L. Glaisher, Esq.

Considering the tone of Mr. Stone's remarks in the *Monthly Notices* for November (vol. xxxiv. pp. 9-15), I felt myself justified in expressing with frankness what I thought with regard to his views and his criticisms on my own.

It was suggested to me that the paper containing these opinions—necessarily somewhat personal—should be withdrawn, and I was advised that the matter rest where it was, without further comment, a course which is, perhaps, the better. I have stated what my belief is in the *Monthly Notices* for April 1873 (vol. xxxiii. pp. 391-402); and have now only to ask the reader not to assume that I hold, because I offer no reply, all the opinions attributed to me by Mr. Stone, *e.g.* in reference to the investigation on pp. 14 and 15. I never held that the *h* of every observation was *à priori* equally likely to have any value from 0 to ∞ , as I took the usual result as a first approximation. I will also ask any one who wishes to form an opinion on the subject in question to read Mr. Stone's paper in the *Supplementary Number* (vol. xxxiii. pp. 570-572), as well as that already referred to.

Second Paper on the probable Variability of some of the Red Stars in Schjellerup's List (Astronomische Nachrichten, No. 1591).
By J. Birmingham, Esq.

(Communicated by Mr. Dunkin.)

At the last December meeting of the Society I had the honour of laying before it an account of the disappearance of No. 252 in the above Catalogue; and, while noticing certain pos-

sible grounds of doubt respecting the reality of the original discovery of that star by Sir John Herschel, I argued against the probability of his making the double mistake of regarding Bessel's adjacent red star as a new one, and also assigning to it a wrong position. It was, therefore, with no little satisfaction that I saw by Mr. Chambers's paper read at the January meeting of the Society, that he observed the star in 1870, together with the neighbouring star, 251, and I have lately found that 252 was seen and spectroscopically examined by Secchi on July 17, 1868; so that it seems abundantly proved that 251 and 252 were distinct objects, and that the latter has recently disappeared.*

I would now direct attention to the probable variability of other stars in Schjellerup's list, which are now convenient for observation.

I first searched for No. 90, on February 3, 1873, and failed to find a red star in, or near its position. I was equally unsuccessful on February 6 and 8, on September 23, and December 26; and on December 28 I wrote to Messrs. Dunkin and Lynn, of the Royal Observatory, requesting that it might be looked for. This was accordingly done, and Mr. Downing, who made the observation with the Transit-circle, found, on January 3, that the exact position of 90 was occupied by a 7-magnitude colourless star without even a tinge of red. It was afterwards identified as one of Flamsteed's stars, 44 *Cameloparduli*. This star, which I knew, appeared to me rather less than the 7th magnitude and of a bluish white colour. Schjellerup notes it as one of the stars in the Dorpat Catalogue, where it is rated at the 7th magnitude and marked *rubra*; so that it seems to be a variable at least in colour, if not in magnitude.

No. 101 is taken by Schjellerup from a note by Piazz, who describes it as red, without stating the magnitude, and rather roughly gives its position as preceding by about 2' another of his stars—No. 187—on the same parallel. I failed to find the red star on February 6, 1873, and was informed by Mr. Knott, to whom I communicated my failure, that he was equally unsuccessful on two previous occasions—on June 25 and August 3, 1866. However, on January 13 of the present year, I found a star in the position of 101, as well as I could identify it in a hurried observation. The passage of clouds did not permit me to examine it very closely, but it appeared about the 8.5 magnitude, and slightly tinged with red. I have not observed it since then; but I would suggest that it should be closely watched in

* Professor Argelander remarks (*Astr. Nach.*, No. 1977) that this star was looked for at the Bonn Observatory on February 26, 1858, with the heliometer, and on November 18, 1859, with the meridian-circle, but on neither occasion was it visible. He is of opinion that there is some error in Sir John Herschel's observations; and Professor Schönfeld expresses the same view, suggesting that though Sir John speaks of "two observations," they may not have both included determinations of place. On the other hand, we have the observations of Mr. Chambers detailed in the January number of the *Monthly Notices*, p. 104. The point appears an interesting one.—[EDITOR.]

its present excellent position for observation, when its very probable variability might be determined.

In No. 74 I have remarked differences within the limit of one magnitude, with considerable changes of colour: Schjellerup thus notes it—*Bessel, roth, Cape Obs., vivid red, 8.*

My observations of it were as follow:—

1871, March 8. Pale red, 8.

1872, Feb. 10. Orange, 8.

1873, Feb. 2. Orange, 7·5 to 8. *A comes* 9·5; intense blue; 285°.

1873, Dec. 20. Fine orange red, 7.

1874, March 6. Fine orange red, 7.

Recently, the angular distance of the comes was found to be 84"; position 288°.

No. 77, described by Schjellerup as *roth*, 7·7, seems less than 8, with the palest possible red tinge.

It may not be out of place here to refer to a very remarkable orange star which I found when looking for 77. On February 2, 1873, it appeared about the 7th magnitude, with a comes of the 11th; position 95°. On December 26 the large star seemed of the 6th magnitude. On February 3 it looked nearer the 5th, and on March 8, it appeared of the 6th magnitude again. Its roughly approximate place by equatoreal is R.A., 6^h 14^m 40^s; Decl. — 2° 54' 30". Its small apparent changes may, indeed, be due to errors of estimation; but I consider it worth the attention of observers, as it is strikingly superior in depth of colour to very many of the stars in Schjellerup's Catalogue; and the fact that it was missed by previous observers of red and orange stars is strongly suggestive of variability. From recent measures, I find that the angular distance of the comes is 100", and the position 99°.

No. 63, unless I mistake its identity, presents an instance of complete change of colour. It appears as one of Schjellerup's own stars in the list, where it is marked "*roth*," but to me it always shows a fine blue tint, while 51 *Orionis*, near it, is deeply orange.

No. 152, which is described in the Catalogue as a red star of 8·5 magnitude, has been observed by me on several occasions as a fine orange star of the 6th magnitude.

No. 280 furnishes an undoubted instance of change. It is catalogued as one of Argelander's stars of the 6th magnitude, and followed by a blue star on the same parallel. On April 18, 1873, I noted it as a good red, but no more than 7·5, and estimated the blue star at the 9th magnitude. On January 15, 1874, the red seemed only of the 8·5 magnitude, and it was considered even smaller by the Rev. Mr. Webb, on the 12th. On February 17, I thought the star had risen to the 7th magnitude, with a high colour; and, in an unfavourable observation

near lower transit, on February 27, I estimated it at only the 8th magnitude. The small blue star near it seems always about the 9th magnitude, and affords a convenient object for comparison. I have just been informed that Secchi saw 280 of about the 8th magnitude, and suggested its variability, which may be regarded as certain; and it now remains to discover the period.

Notes on the Zodiacal Light. By the Rev. Samuel J. Johnson.

What Humboldt speaks of as the "mild pyramidally shaped zodiacal light, very visible to the unassisted eye," has been displayed here this winter with far more distinctness than I have noticed since February 21, 1870, when I witnessed a vivid appearance of the phenomenon from Lytham on the Lancashire coast.

It was conspicuous, amongst other nights, on February 8, when the impression that Tycho mistook the Light for the "abnormal vernal evening twilight," appeared at first sight almost pardonable.

February 16. Sky clear for a brief interval about 8 P.M. The conical figure very fairly defined, except at the apex, where the curvature was somewhat difficult to make out. *Mars* situated nearly on the axis; about which point, the Light seemed equal in brightness to that portion of the *Milky Way* that passes through *Cassiopeia*. Nearer the horizon, the intensity was decidedly greater, ν *Ceti* appeared just outside the cone of light; the head of *Aries* faintly involved in it. It could be traced, though with difficulty, 3° or 4° above the *Pleiades*.

February 18. Could readily be followed before the Moon set. Boundaries, so far as could be made out, the outline being indistinct, on the left θ , ν , μ *Ceti*, λ *Tauri*, then passing round the *Pleiades*, about which region the figure was very faint. On the other side δ and α *Arietis* were just outside it. From this point the boundary line would have to be drawn through the 5th mag. stars ψ and τ *Pegasi* to the horizon. Clear extent at the base 30° to 35° . Not quite so brilliant as on the 16th. I fancied a slight reddish tinge in the brighter portions.

February 19. The Moon did not set till $9^h 14^m$, when the more conspicuous parts had descended below the horizon.

An interval of moonlight nights followed.

March 6. The Zodiacal Light again conspicuous. In extent and general features unaltered; in intensity scarcely so great. The clearest defined portion lay between ν *Ceti* and γ *Arietis*; at lower altitudes, the Light, although brighter, appeared very much diffused. *Mars* about 5° left of the axis.

March 7. With regard to the earliest visibility of the Light,

it was not noticeable till 15^m after stars of the brightness of γ *Arietis* had shone out, and not quite so soon as the *Milky Way* at equal altitudes. Its whiteness more dusky than the latter. At an altitude of about 20° η and α *Piscium* (the latter just within the boundary) were somewhat dimmed by its intensity.

Upton Helions Rectory, Crediton,
1874, March 9.

Note on the Zodiacal Light. By E. B. Knobel, Esq.

I would beg to direct attention to the unusual brilliancy of the Zodiacal Light this winter.

It is stated in text-books that the Zodiacal Light is only visible in the evenings, in this country, in the spring months; and I have repeatedly observed it in March and April, but have never seen it more brilliant than in January and February of this year.

On two clear evenings in the first week in January, on January 17, at 6 45 P.M., and lastly on February 8, at 7 P.M., it appeared as an elongated luminous cone, the apex of which on January 17 extended nearly to the star γ *Arietis*, and on February 8 the apex just enclosed η *Piscium*.

It appeared nearly as bright as the *Milky Way*, and sufficiently bright to attract the attention of a casual observer.

I should mention that my situation is quite away from the town, and sufficiently high to be above the mists of the valley.

Stapenhill, Burton-on-Trent,
1874, Feb. 11.

On the Structure of the Solar Photosphere. By S. P. Langley, Esq.

(Communicated by Mr. Lockyer.)

During some years past the spectroscopic investigation of the solar surface has furnished new and valuable results so continuously, as to engross the attention of observers almost to the exclusion of older methods. I have been led, however, to think, that much of interest remains to be reached by direct telescopic scrutiny of the Sun, and that among the facts thus to be gathered are some concerning which the spectroscope cannot inform us, and some which will aid us to interpret its indications.

It will be remembered that an interesting controversy arose

out of the announcement of Mr. Nasmyth's "willow-leaf" structure; and that, after eliciting valuable communications from Messrs. Dawes, Lockyer, and others, it may be said to have closed with a paper by Mr. Huggins, which appeared in the *Monthly Notices* for May 1866. Since this, with the exception of some observations of Padre Secchi, little of essential importance has been added to our knowledge of the intimate structure of the photosphere. As the nomenclature of this subject is in complete confusion, it is necessary to call attention to the fact, that I here mean by "rice-grain" exactly what Mr. Huggins denotes by "granule," (I quote him here from memory), and that I reserve the latter word for a restricted and distinct use. The word "rice-grain" is descriptive of the appearance of certain components of solar clouds, but has no special value as a simile, save for what is seen with moderate apertures;* such as were employed by Messrs. Stone and Dunkin, who first used the word. By "rice-grains" are here meant certain universal constituents of solar clouds, visible only in fine definition; presenting a precise shape as compared with those larger but vaguer cloud-forms or mottlings which are visible on the photosphere with lower powers; of an irregular outline, frequently rudely oval; whose average dimension of from 1" to 2" is yet sometimes fallen short of, sometimes exceeded; which to ordinarily careful observation seem in close juxtaposition, and separated by narrow intervals which are not uniformly dark; the rice-grains themselves being of vivid, but unequal brilliancy, and in the language of Sir John Herschel, "the immediate sources of the solar light and heat."

With an object-glass of thirteen inches, possessing great perfection of figure, and which has been employed with undiminished aperture by aid of the polarising eye-piece, I was led to notice that the rice-grains were by no means in such close juxtaposition as at first appears; the large aperture, by reducing irradiation, helping to make it more easily visible, that these bodies, though the chief sources of the solar light, occupied but the smaller portion of the solar surface.

In measuring them by the filar micrometer, and in reckoning their number (by the aid of a reticule kindly ruled for me in minute squares on thin glass for this purpose, by Mr. Rogers), I was led to notice that my estimates of both size and numbers varied with the magnifying powers employed, in a way which seemed unaccountable on the supposition that rice-grains were individual things of approximately uniform size. By taking advantage of the brief and rare intervals of definition, which admit the use of the high powers of such a telescope, I found the cause in the resolution of the rice-grains into an order of minuter components, hitherto scarcely observed. These components I here

* Mr. Stone observed the "rice-grains" with the Great Equatoreal at the Royal Observatory, the clear aperture of which is 12·8 inches.—[Editor.]

term *granules* (a word used by others as a synonym for "rice-grains," and to which a distinct and restricted significance is, it should be noticed, here attached). They are very minute bodies present over the whole solar surface, faintly discernible in the faculæ, and in the penumbrae of the spots are extended into long filaments, whose aggregation forms the "thatch-straws" of Mr. Dawes, as the aggregation of granules forms the rice-grain. The latter term should be employed hereafter, I think only so far as it may be necessary to recognise a tendency of these granules to unite in clusters of approximately uniform size. The granules are occasionally seen singly, more frequently united in clusters of from two or three to ten or more ("rice-grains"), and by their degree of juxtaposition, and perhaps by their partial superposition, form the inequalities of brilliancy of the rice-grain noted by Mr. Huggins, and account for the irregular outline of the latter, which he has already remarked upon. With the largest apertures and powers not only then do these brilliant bodies appear smaller, but from their apparent area is to be taken the minute dark spaces which it now appears intervene between their own component parts.

It hence became a matter of interest to determine approximately the average number of granules to a given area, and their size, since it was increasingly evident that the part of the solar surface which is principally concerned in sending us light is much smaller than has been supposed.

The general background on which the granules appear is grey by comparison, not uniform in shade but faintly mottled, and its light is very probably also due to the presence of granules, partly dissipated or concealed.

We are not now concerned with this further than to remark that, if this be so, it does not affect the following estimates of the brilliant area, which rest upon the fact that though granules and mottled interspaces here and there merge into one another, there is yet between these ultimate constituents of solar clouds and their background, a distinction so great that we are warranted (for the purpose at least of a first classification), in treating the solar surface as though it were composed of uniformly bright granules, divided by uniformly grey intervals in which the light is certainly so inconsiderable that the granules appear brilliantly irradiant.

In our measurements, let us first remark, that owing to the vivid irradiation of these bodies, a conclusion as to their real size from their apparent angular dimension would be nearly as unwarrantable as a similar estimate would be in the case of a bright fixed star. To evade this difficulty altogether seems impossible, yet I believe the course adopted fixes an upper limit for their apparent size. It has been remarked that the granules are continuous with certain long filaments in the penumbrae; and these filaments, though often much contorted, are in the normal type of penumbra radially disposed, and closer as they converge

toward the centre. By repeated measurements with the filar micrometer, and by other means, it was found that the distance of these filaments varied widely, but that where they lay closest their centres were certainly less than $0''.6$ apart. As it may be safely assumed that two bright lines separated by an interval of less than $0''.3$ would appear as one in the instrument and with the power employed, it appears that the interval between the filaments observed at least equals their width, which cannot in this case be more than three-tenths of a second of arc, and which, not improbably is much less. But filament and granule are apparently different aspects of the same thing, the granules being the upper end of filaments disposed, in this view (which is partly Mr. Dawes's) in a general sense vertically over the Sun.

The granule being certainly not materially larger than the filament at its extremity, we cannot consider it as over $0''.3$ or $0''.4$ in diameter. I feel increased confidence in the result of these difficult measurements, from the fact that Secchi, who is the only observer who appears to have seen them, speaks of minute objects he terms *grains*, as covering the whole solar surface, and whose diameter he estimates at from a third to a quarter of a second of arc. These, are doubtless, identical with the objects here termed granules, though they are treated by Secchi as identical with the "rice-grains" of Stone, and the "willow-leaves" of Nasmyth, with neither of which will their size permit us to confound them. Having the size of the granule at its upper limit, their number is obtained by the aid of the Rogers-reticule and by actual count. This is a work of extreme difficulty. I find that if the granules were uniformly distributed we should have rather less than one to each second of arc; but I should here state, that I have reason to believe it probable that neither granules or filaments are "entities" in any other sense than "rice-grains" are. Filaments of immeasurable fineness are at times seen projected on the umbræ, looking, collectively, like carded wool, which are intimately connected with those measured; and though it would not be right to ignore the fact of a tendency to certain more or less definite groupings, which have a real existence, and to which we give here such names as "granules" or "rice-grains," according to their order of magnitude, we must not be misled by our own nomenclature to claim for any of these an objective reality in the sense at one time attributed to the "willow-leaves." There is no improbability then that the division will be hereafter carried further; but it is essential to note that as the increase of the number will imply a proportionate diminution of size, we have, if our present estimates of the relation between size and number be correct, already data which will enable us to fix an upper limit of the ratio of the proper illuminating surface to that of the whole photosphere, which cannot be disturbed by subsequent minuter discrimination. I do not feel that the data given are exempt from considerable possible error, owing to the great rarity of the occasions when they can be gathered or verified; but after making

every reasonable allowance for this, we cannot reach the result of our computation without surprise, for it leads inevitably to the conclusion that the Sun's light comes chiefly from an inconsiderable portion of its surface, and I cannot myself estimate this portion at more than one-fifth of the whole.

I have been led to think that the study of the forms assumed by the filaments will give information of interest as to the character and direction of the currents in the photosphere, whose existence Mr. Lockyer and others have already demonstrated by the spectroscope. These may be studied to most advantage in the penumbra. The darker shade, around the outer penumbral edge, is, I observe, ordinarily only that of the grey matter everywhere on the surface, serving as a background for the granules; and the evidence from a prolonged study of the minute structure here, seems to show that this edge is formed by rupture, by a stress that is, acting laterally and downwards, and not by a deposition of the constituents of ascending currents.

These filaments are not only more brilliant at the umbral edge (as if their extremities were curved upward, and less obscured, where partly elevated above a darker supervening medium), but this tendency may be traced in good definition, all over the penumbra, in which they have a certain tendency to unite in narrow sheets or plates, which, superposed, form the fascicles called "thatch-straws" by Mr. Dawes. In the umbra, the detached bright points, observed by Mr. Lockyer and others, are found to be tips of filaments thus raised, the part of the filament extending under the umbral shade being frequently, by proper precaution, dimly, but unequivocally traceable through it, and found continuous with a portion of the penumbra. By the aid of the polarizing eye-piece, and by excluding as far as possible extraneous light, the whole umbra is seen at times to be nearly or wholly made up of sunken banks of the filaments; the umbral structure being quite complex, and presenting the appearance of a submerged penumbra, and the nuclei of Mr. Dawes, being only deeper portions of its shade.

By means, for the detailed description of which there is not here space, I have isolated one of these nuclei from the surrounding umbra. Mr. Dawes terms the nucleus *intensely black* (the italics are his), but thus viewed it is not "black," not even dark, except in a relative sense, but brilliant with a violet-purple light. I regret that I have been unable to obtain, as yet, photometric measurements of the luminosity, but when seen alone it might fairly be described as dazzlingly vivid, the appearance being like that of a violet star, and very striking. The observation is one which may have been already made; yet I have not seen any determination of the light of the nucleus as distinguished from the umbra, and its interest in connection with the disputed question of the transparency of the Sun's interior induces me to give it a place here.

I have never been able to satisfy myself by direct telescopic

scrutiny, whether these "banks" in the umbra have an invariable direction of motion, whether of ascent or descent. Nearly all my observations have tended to confirm me in the impression that they moved as if under the direction of a downward current; yet in one instance I have seen the contrary, and in this case the evidence of ascent seemed unequivocal. Mr. Lockyer (*Monthly Notices*, June 1865) has noticed in one instance the fact of the existence of superposed currents moving in different directions. I think this nearly isolated observation may be greatly extended; and I can, from what I have repeatedly seen, entertain scarcely more doubt of the frequent superposition of approximately horizontal currents of the different strata of solar clouds than of our own.

What may be almost called the typical form of solar currents, the cyclonic, has been left unmentioned. Of the various kinds of this as traceable by the disposition of the flexible filaments, I have made a very considerable number of sketches which were in every case finished at the telescope, and though inartistically, yet, I believe, not unfaithfully represent the forms seen there. Any complete discussion of these would be impossible here, but it may be stated generally—

That the normal type of cyclone contemplated by M. Faye as embracing the whole spot in a common movement of rotation is extremely rare.

That the evidence of cyclonic action is nevertheless everywhere present in the spots. The radius of curvature of the filaments diminishes rapidly to the centre of the whirls, which are very numerous in large spots; often completely independent when in apparent juxtaposition, or even apparently in part superposed, and sometimes rotating in different directions.

That the axes of these whirls are not always vertical, but are occasionally met with inclined at all angles.

That the filaments in incipient, small, or growing spots are less frequently seen moulded by any uniform cyclonic action than in forms whose disposition is nearly that seen in processes of crystallisation, or gemmation.

Padre Secchi almost alone has given some illustrations of these "crystalline" forms, and it is to be wished he had given more. I have myself seen some so unlike anything we should be prepared to expect from the ordinary published illustrations, that I should be glad to have the independent evidence of so eminent an observer to refer to in making them public. I can only say here, and in the absence of special illustrations, that this crystalline appearance is very remarkable not only in itself, but from its contrast with the flexible forms the filaments ordinarily assume, and that the most peculiar types are confined to large spots, such as at the present time rarely present themselves.

Both in their points of resemblance to, and distinction from ours, however, the solar clouds are of interest to the terrestrial

meteorologist; but many more observers are wanted in this direction than we have at present, and it would be especially desirable if photography could do for the details of spots what it has already done for the laws of their movement. Our own atmosphere is the great obstacle, and a more serious one in observations by day than by night, as weeks may pass without bringing an hour of definition suitable for the best use of the very large apertures indispensable for some of the observations which have been detailed, and their demand upon the time of the professional astronomer having other duties is frequently too great to be met. I should hardly have felt justified in commencing observations of this kind had I known the assiduous attention they require in awaiting what are in our climate the rare moments when they can best be made. I shall feel less regret for the disproportionate time thus spent, if the present communication should be at all instrumental in directing the attention of those private observers now possessing powerful instruments of a class comparable with that used to a most interesting field of research, as yet scarcely occupied, and in which their labours may be instrumental in laying the foundations of a future science of solar meteorology.

*Allegheny Observatory,
Allegheny, Pennsylvania, 1874, January 30.*

On the Approximate Calculation of the Times of Solar Eclipses.
By J. N. Lewis, Esq.

(*Extract from a Letter to Mr. J. W. L. Glaisher.*)¹

In the *Monthly Notices*, Vol. xxxii. p. 332, is an article by the Rev. S. J. Johnson, "On Future Solar Eclipses." In calculating these he makes use, for brevity, of the Tables contained in the eighth edition of the *Encyclopædia Britannica*, and he says he finds only one instance in which they are more than nine minutes out in the time of greatest obscuration. This recalls to mind that I prepared, a few years ago, a set of Tables of still greater brevity than those in the *Britannica*, and, according to the testimony of Mr. Johnson, more accurate also. As to this latter point, some results are given in the following Table. The Table of Epochs was prepared from the elements of the Moon's orbit given by Hansen (*Tables de la Lune*) and of the Sun's orbit by Le Verrier (*Annales de l'Observatoire de Paris*, tome iv.) taking into account the secular equations; and the equations to reduce the mean to the true syzygy were computed from the formula in Burekhardt's *Tables de la Lune* (Paris, 1812) p. 87, two small Tables being added for the purpose of taking into account the "Reduction," and the change in the velocity of the Moon's motion between the mean and the true syzygy; also for the Moon's latitude,

parallax, &c. Very brief Solar Tables are also added. These Tables were intended to be used, in connection with a set of diagrams, or graphical constructions, for making a rapid examination of all the recorded ancient eclipses. They will give a pretty satisfactory representation of the circumstances of any eclipse from B.C. 720 to A.D. 2000, and thus save a vast amount of calculation that might otherwise be wasted upon eclipses that would turn out to be of no importance. They would thus serve as a criterion to indicate or point out the cases where exact calculations might be resorted to, without the risk of losing the labour and time expended. Among others, I intended to apply them to an examination of the Chinese eclipses, lists of which are given by Mr. Williams in Vol. xxiv of the *Monthly Notices*; but a press of other business has caused me to lay aside the whole matter until I shall have more leisure.

I send you the following comparisons. The results from the approximate Tables were, in each case, obtained by a few minutes of calculation. The numbers in the columns headed "True time" and "True latitude" are the results derived from extended Tables:—

Approx. Time of Syzgy.				True Time.				Discordance.	Moon's Latitude.		
	d	h	m		d	h	m		Approx.	True.	Discordance.
1831 Feb. 12	5	14		12	5	13		+1	+42		
1832 July 27	2	1		27	2	2		-1	+4	+3	+1
1834 Nov. 30	6	45½		30	6	47.7		-2	+53	+51	+2
1836 May 15	2	7		15	2	7		0	+26	+26	0
1838 Mar. 25	9	45		25	9	45		0	-47	-46	-1
1838 Sept. 18	8	45		18	8	45		0	+47	+48	-1
1842 July 7	19	0		7	19	1		-1	+28	+28	0
1844 Dec. 9	8	12		9	8	13		-1	+72	+72	0
1846 Apr. 25	4	48		25	4	48		0	+11	+12	-1
1850 Aug. 7	9	34		7	9	33½		+½	0	+1	-1
1851 July 28	2	41		28	2	40½		+½	+45	+46	-1
1854 May 26	8	46		26	8	47		-1	+23	+21½	+1½
1858 Mar. 15	0	13		15	0	12		+1	+38	+38	0
1860 July 18	21	19		18	2	19		0	+34	+33	+1
1865 Oct. 19	4	27		19	4	26		+1	+30	+29	+1
1868 Feb. 23	2	19		23	2	20		-1	+5	+4	+1
1869 Aug. 7	10	6		7	10	7		-1	+41	+42	-1
1871 Dec. 11	16	1		11	16	1½		-½	+11	+11	0
1878 July 29	9	39		29	9	39		0	+38	+37	+1
1900 May 28	2	48		28	2	48		0	+23	+23	0
1905 Aug. 30	1	11		30	1	11		0	+35	+35	0

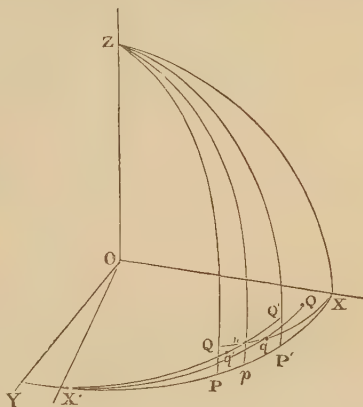
The parallaxes, hourly motions, &c., are given with a like degree of approximation.

*Mount Vernon, Ohio, U.S.,
1874, March 2.*

Note on the Curvature of Lines in the Dispersion Spectrum, and the Method of Correcting it. By W. H. M. Christie, Esq.

When a spectrum is formed in the ordinary way with a spectro-scope, the lines are curved, and no adjustment of the prisms will get rid of this serious defect. This point has, I believe, not been investigated, though there is no great difficulty in the problem, in the simple form it assumes when the pencil, passing through the prism, consists of parallel rays—a condition which ought always to be satisfied in a properly adjusted spectro-scope. It will in this case be sufficient to consider the directions of the rays, in order to show that the image of the slit, after refraction through a prism, will be curved.

Taking the plane of yz for the face of the prism on which the rays are incident, OZ for the refracting edge, and OX the normal to the face, let PO, QO be the directions of pencils from the centre and one extremity of the slit respectively, pO, qO the directions of the corresponding refracted pencils, n the intersection of the arcs Zp and QX , and let $\angle PXQ = \alpha$,



Then

$$\frac{\sin QX}{\sin qX} = \frac{\sin PX}{\sin pX} = \mu,$$

and

$$\frac{\cot nX}{\cot QX} = \frac{\cos \alpha \cdot \cot pX}{\cos \alpha \cdot \cot PX} = \frac{\cot pX}{\cot PX}$$

by the right-angled spherical triangles PQX, pqX ;

$$\therefore \frac{\cos nX}{\cos QX} \cdot \frac{\sin QX}{\sin nX} = \frac{\cos pX}{\cos PX} \cdot \frac{\sin PX}{\sin pX} = \frac{\cos pX}{\cos PX} \cdot \frac{\sin QX}{\sin qX}$$

$$\therefore \sin nX = \frac{\cos PX}{\cos pX} \cdot \frac{\cos nX}{\cos QX} \cdot \sin qX;$$

but

$$\cos nX = \cos pn \cdot \cos pX$$

and

$$\cos QX = \cos PQ \cdot \cos PX$$

$$\therefore \sin nX = \frac{\cos pn}{\cos PQ} \cdot \sin qX \sim \sin qX,$$

or,

$$nX > qX.$$

Therefore, the image of a straight line after the first refraction is concave towards the normal to the first face. The amount of this curvature may readily be found, as follows :

From the last equation—

$$\frac{\sin nX - \sin qX}{\sin nX + \sin qX} = \frac{\cos pn - \cos PQ}{\cos pn + \cos PQ},$$

or,

$$\frac{\tan \frac{1}{2}(nX - qX)}{\tan \frac{1}{2}(nX + qX)} = \tan \frac{1}{2}(PQ - pn) \cdot \tan \frac{1}{2}(PQ + pn),$$

which gives, since $nX - qX$, PQ and pn are small quantities,

$$nX - qX = \frac{1}{2}(PQ^2 - pn^2) \cdot \tan qX \text{ nearly;}$$

but

$$\sin PQ = \sin \alpha \sin QX,$$

$$\sin pn = \sin \alpha \sin nX = \sin \alpha \sin qX \text{ nearly}$$

$$\therefore \frac{pn}{PQ} = \frac{\sin qX}{\sin QX} = \frac{1}{\mu};$$

Hence

$$nX - qX = \frac{\mu^2 - 1}{2\mu^2} \cdot PQ^2 \cdot \tan pX \text{ nearly.}$$

For the refraction, on emergence, let OX' be normal to the second face of the prism, $P'O$ the direction of the emergent pencil (incident in the direction PO), $X'nQ'$ an arc of a great circle: then, if we suppose the direction of the emergent pencil reversed, the ray OQ' would be refracted in the direction Oq' (q' being a point on the arc nX'); and therefore the pencil which passes through the prism in the direction Oq will emerge in the direction OQ , $X'qQ$, being an arc of a great circle, and $QX' > Q'X'$; therefore, the image on emergence will be concave towards the normal to the surface of incidence and its curvature, after refraction through the prism, will be—

$$\frac{\mu^2 - 1}{2\mu^2} \cdot PQ^2 (\tan pX + \tan pX'),$$

where pX' may be negative, but is always numerically less than pX , since the deviation is always from the edge of the prism.

This expression will be a minimum when the pencil passes through the prism with minimum deviation, for let $pX = \phi'$ and ι = angle of prism,

$$\begin{aligned} \tan pX + \tan pX' &= \tan \phi' + \tan (\iota - \phi') = \frac{\sin \iota}{\cos \phi' \cos (\iota - \phi')} \\ &= \frac{2 \sin \iota}{\cos \iota + \cos (2\phi' - \iota)}, \end{aligned}$$

which is a minimum when $2\phi' = \iota$.

Hence the curvature cannot be got rid of by any adjustment of the prism, and increases with every prism used.

But if, after passing through the train of prisms, the rays be reflected directly back again by a plane mirror, so as to pass through the collimator, and form an image slightly on one side of the slit (which image may be viewed by means of a diagonal prism), the course of the pencil being reversed, the curvature will be corrected by the second passage through the prisms, and the dispersion will be the same as if the pencil had passed through a train, formed by the prisms themselves and their reflections in the plane mirror.

This is done in the Spectroscope now being made for the Royal Observatory, and the lines are perfectly straight.

When the ordinary right-angled prism is used at the end of the train, there is an inversion with regard to up and down which prevents any compensation of this kind from operating.

Blackheath,
1874, *March 10.*

On a Method of drawing, by continued Motion, a very close Approximation to the Parabola, proposed with a view to its possible Application in figuring Reflectors. By F. C. Penrose, Esq.

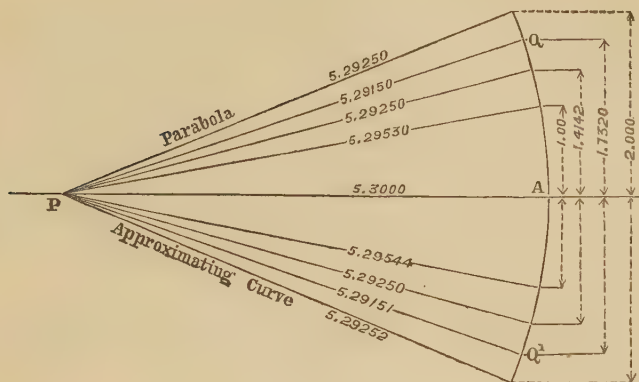
Professor Sylvester, in his Lecture at the Royal Institution in January, showed how two of Peaucellier's parallel motion cells might be combined so as to draw the Conic Sections.

This instrument, however, when arranged so as to draw the Parabola (which it does with theoretical exactness), owing to its working at a mechanical disadvantage, could hardly be made available in practice where steadiness is of the utmost importance.

Another application of the instrument, of which a working model is produced, moves remarkably steadily, and although the curve drawn is not the Parabola in theory—for it is a line of the third order—yet, for a sufficient arc for the majority of optical purposes, may be made to coincide with it so accurately as not to vary from it more than—say, a three hundred-thousandth part of the aperture in a mirror intended for a telescope. I am not speaking of errors of execution, but of theoretic divergence, and various contrivances could be applied to make the execution very exact.

The case which I give in Fig. 1 supposes a reflector of very short focus, not exceeding four times the aperture.

Fig. 3.



Differences of Radius Vector.

+ '00008

+ '00001

± '0

± '00014

± '0

The two curves are put together at the vertex, and also are adjusted at Q or Q', where they have a common normal, the radii of curvature having also very small difference.

The abscissæ at the places compared are 0.1, 0.2, 0.3, 0.4.

The polar equation to the curve is $\frac{m+\mu}{m} c \cos + \theta \frac{m}{c} \sec \theta = \zeta$

where m and μ are the difference of the squares of the lengths of the links forming the cells of the instrument, and c is the diameter of the circle upon which it is made to revolve.

Wimbledon,
1874, March 12.

Notes on some Spectroscopic Observations of Sirius, γ Argûs, &c.
By E. H. Pringle, Esq.

(Communicated by Capt. J. Herschel, R.E.)

The following observations were made at Mangalore, South Canara, with a Spectroscope of two prisms of dense flint glass, with refracting angle of 60° , attached to a $6\frac{1}{2}$ -inch silvered-glass speculum telescope.

Most of the observations have been made after removing one

of the prisms, and replacing the short slit by one of greater length, for it was found impossible to keep the image of a star in view with a small field owing to the telescope having altazimuth motion. The measurements, which must only be regarded as approximate, were taken by the angular motion of the small observing telescope.

The eye-piece generally used has a power of about 11.

I first examined *Sirius* on the 11th inst., removing the slit and using a cylindrical lens, and noticed a band in the violet, which I do not find chronicled. Subsequently on the 18th, a night of remarkable clearness, a still more refrangible line was visible, and beyond this again an extent of violet light. On the 19th, and two succeeding nights, after fitting a new slit to the instrument, measurements of these lines were obtained.

A noticeable point in this star's spectrum is the brilliance—comparatively speaking—of the violet light. It argues an absence of the numerous lines and bands which mar this part of the solar spectrum.

The lines x and y appear very broad, much more so than the hydrogen lines in the star. A cylindrical lens was occasionally used; but for the purpose of measuring I found keeping the observing telescope very slightly out of focus enabled the lines to be distinctly seen, whilst there was less loss of light. Instrumental parallax was guarded against as far as possible, but as before noticed the measurements are only approximate.

γ *Argús*. This is the star whose bright line spectrum was observed in December 1871, by Professor Respighi. In January 1872, ignorant of the above discovery, I accidentally hit upon this star, and examined its spectrum with a small direct vision spectroscope, attached to an achromatic of 3-inches aperture.

The three principal bright lines, two in the yellow and one in the blue, were very distinct; and I suspected a fourth still more refrangible than the yellow lines.

I have now re-examined the star, and find the suspected line has an existence. There is, moreover, a fifth line or band near to, and less refrangible than the blue line. This band seems to be defined on the blue, but to fade off gradually towards the red.

I suspect either bright lines or absorption bands in the orange, from the unequal brightness of this part of the spectrum.

Zodiacal Light. The many observations I have taken of this phenomenon have failed to elicit more than a faint diffuse spectrum. I have had—and fear that this season I shall have—no opportunity of examining the Light from the elevation of 6,000 feet, as I had hoped to do.

The Earth-light from the Moon when crescent gives a spectrum terminating abruptly at a point very little less refrangible than E , but fades off gradually in the blue, and is traceable to somewhere about $2250\ K$.

Margalec,

1874, January 27.

The Nebula near η Argûs.

The following is an Extract from a Letter addressed by R. J. Ellery, Esq., to Warren De La Rue, Esq., dated Melbourne, 1874, January 28:—

“We have commenced another drawing of η Argûs. It is again quite different from what it was six months ago; the great bay of the lemniscate is nearly filled up with dense nebula leaving a dark sigmoid inlet, which appears *inky* by contrast. Moderately dense nebula has formed from side to side of the opening where η is situated, embaying and apparently about to involve that star, so that η and the lemniscate are at present in a *lake* (!) completely surrounded by nebula.”

On a Determination of Longitude by Chronometers, by the late Lieut. Hastings F. Murphy, R.E. By Robert J. Lecky, Esq.

I have much pleasure in presenting to the Society an original calculation of longitude by chronometer comparisons, made by the late Lieut. Hastings F. Murphy, R.E., astronomer to Colonel Chesney's Euphrates Expedition, who died of fever at Bussorah on the 6th of August 1836, immediately after the successful completion of his arduous duties, and of whom we have an interesting memoir in vol. x of the *Memoirs* and in vol. iv. of the *Monthly Notices*, written by the late Mr. Francis Baily.

Lieut. Murphy held an appointment in the Ordnance Map Office in the Tower; and early in the year 1832 I had a request from him to have correct time ready for him at the Observatory of the Royal Cork Institution, then under my care. He was on leave, and about to visit his father, Rector of Kiltallagh in the county of Kerry, and was bringing with him his own pocket chronometer and two others, which were carefully rated prior to leaving London, as well as while he was at Kiltallagh; and when passing through Cork, both on his outward and return journeys, had good comparisons with me; and on his return to town, sent me the accompanying calculations, which are valuable as a carefully worked-out example of such observations, as well as a memento of a most talented and amiable officer.

He also had a comparison with the late Dr. Dartnell, at Youghal, when passing through that town by coach, but this was only made on one journey, and I also fear that Dr. Dartnell was not sufficiently exact as to the error of his chronometer, or that Lieut. Murphy may not have taken it down correctly as our well ascertained local difference was $2^m 28^s$, while this shows it as $2^m 48^s$. Notwithstanding this error, I leave this calculation attached to that for Cork, and I accompany them with the one-inch scale Ordnance Maps of the places, which give the latitude and longitude with great accuracy. The cross lines in red on these maps show the position of both Observatories; that for

Youghal shows longitude $7^{\circ} 51' 10''$, or in time $31^m 24^s.40$. Lieut. Murphy makes it $31^m 2^s.79$; difference $21^s.61$, which is a manifest error, and particularly so as the differences for each chronometer correspond very closely. The Cork map gives a longitude of $8^{\circ} 28' 12''$, or in time $33^m 52^s.48$; Lieut. Murphy's being $33^m 52^s.74$; the difference being $0^s.26$, which is much more likely to have been my error than his, and is, I fancy, a very close result, considering the small number of chronometers employed, and the rougher modes of conveyance in that day compared with those with which we are favoured in the present.

Box Chronometer. "French," No. 3179.

1832	Greenwich to Cork.	Cork to Greenwich.	
	m s	m s	
Mean daily rate . . .	— 0 0'55	+ 0 0'57	
Elapsed time . . .	0 3'729	0 4'587	
	— 0 2'051	+ 0 2'61	
Civil reckoning.			
Jan. 30. 15½ S. of G. . .	— 0 27'6	— 0 46'5	March 18
	— 0 29'651	+ 0 49'11	
Feb. 3. 9 F. of C. . .	33 19'10	33 6'83	March 14
	33 48'75	33 55'94	Mean m s
Pocket chronometer, 1894	33 58'50	33 44'73	Do. 33 51'62
Do. 156	33 59'27	33 48'55	Do. 33 53'91
	33 55'51	33 49'74	Do. 33 52'63

Or, mean of 1st chronometer, with mean of 2nd and 3rd . . . 33 52'55
 Mean of both journeys, giving value to each inversely as elapsed
 time, viz., G to C, $-3'729$ days, and C to G, $4'587$ do. } 33 52'93

Mean longitude of Cork Observatory, given as nearest to the truth 33 52'74 W.

		Longitude of Youghal.					
		m	s	m	s	m	s
1st, No. 3179	=			2	48'88	} 2 48'95 33 52'74	Youghal and Cork Cork and Greenwich
2nd, „ 1894	=	2	48'73	2 49'02			
3rd, „ 156	=	2	49'30				

Dr. Dartnell's chronometer slow, which
 must be in error — 0 1'00

31 3'79

31 2'79

Observation of Faye's Comet (Comet VI. 1873.) By Dr. C. H. F. Peters.

As there have been no observations of Faye's Comet published, except those made at Marseilles, I have to communicate the following position, the only one that I have obtained :—

	Mean Time Hamilton Coll.			R.A.			Decl. "		
	h	m	s	h	m	s	°	'	"
1873, Dec. 23	14	10	29	9	18	19.68	-2	15	4.4

The Comet was observed with a ring-micrometer, and the observed place is the mean result of twelve comparisons with a Star whose position is well determined. It was necessary for the air to be very pure to allow the Comet to be seen, and consequently the observation is not so satisfactory as we could have wished. However, the comparison of the observed place with the ephemeris of Dr. Axel Möller gives the following differences :

Calculation—Observation.

$$\text{R.A.} = -0^{\circ}37 \quad \text{Decl.} = +16''.5$$

which confirms the testimony of the observations at Marseilles as to the general accuracy of the ephemeris of Dr. Möller. (*Bulletin International*, March 20, 1874.)

Discovery of Minor Planet (135). By Dr. C. H. F. Peters.

(*Extract from a Letter to the Astronomer Royal.*)

The telegraph will have duly announced the discovery of Asteroid (135) on February 18. Since that date, on account of the weather, I have obtained only one more observation, and have not yet had the opportunity for determining accurately the place of the comparison-star of the first night. The observations are as follow :—

1874	Hamilton Coll. M.T.			R.A. (135)			Decln. (135)			Number of Comp.
	h	m	s	h	m	s	°	'	"	
Feb. 18	14	37	49	11	19	42.7	+ 4	25	5	12
„ 24	13	24	45	11	14	36.47	+ 4	53	3.6	10

The planet was compared on February 18 with an anonymous star of the eleventh magnitude, and on February 24 with W.B. (1) xi. 233. The notice given by the telegram of the planet's motion being nearly parallel to the Equator must be modified, as the daily motion is in reality between four and five minutes north. The magnitude was estimated at 11.2.

*Litchfield Observatory,
Hamilton College, Clinton, N.Y.,
1874, February 26.*

Discovery and Elements of Comet I., 1874.

This Comet was discovered by Dr. Winnecke, at Strasburg, in the night of February 20-21, and the observed approximate place, determined from a comparison with a star of the 9th magnitude in the *Bonn Durchmusterung*, +26°. No. 3954, was found to be—

Mean Time at Strasburg				R.A.			N.P.D.	
d	h	m	s	h	m	s	°	'
1874, February 20	16	0		20	35	0	63	55

From later observations on the same morning, using 27 *Vulpeculæ* as the comparison-star, the place of the Comet was—

Mean Time at Strasburg.				R.A.			N.P.D.			Number of Comp.
	d	h	m s	h	m	s	°	'	"	
1874, February 20	17	16	40.6	20	35	34 10	63	59	15.2	5

Dr. Winnecke observed the Comet also on the two following days, the observed places being—

Mean Time at Strasburg.				R.A.			N.P.D.			Number of Comp.
d h m s				h m s			° ' "			
1874, February 21	17	11	2.7	20	44	29.44	65	25	9.2	6
" "	22	16	31 6.6	20	53	10.58	66	53	21.7	8

The mean places of the comparison-stars for 1874.0 are—

		R.A.			N.P.D.			
		h	m	s	°	'	"	
February 20		20	31	42.06	63	58	31.0	Washington 12-y. Cat.
"	21	20	42	40.97	65	49	28.2	Bessel
"	22	20	54	23.43	66	52	47.4	Arg. vol. vi.

The following elements of the Comet have been computed by Dr. Schulhof, of Vienna, from the observations made at Strasburg on February 20, at Pola and Vienna on February 23, and at Pola and Vienna on February 25—

$$\begin{aligned}
 T &= 1874, \text{ March } 9^{\text{h}} 53^{\text{m}} 42^{\text{s}}. \\
 \pi &= 300 \quad 36 \quad 4.2 \\
 \Omega &= 31 \quad 31 \quad 18.2 \\
 i &= 58 \quad 17 \quad 14.5
 \end{aligned}
 \left. \vphantom{\begin{aligned} \pi \\ \Omega \\ i \end{aligned}} \right\} \text{Mean Equinox, 1874.0.}$$

$$\log. q = 8.642852$$

A comparison with the mean observed place gives the following errors:

$$\begin{aligned}
 \Delta \lambda \cos \beta &= + 3''.1 \\
 \Delta \beta &= - 8''.2
 \end{aligned}$$

Occultations of Neptune and ν^2 Cancrī, observed at Mr. Barclay's Observatory, Leyton. By C. G. Talmage, Esq.

Occultation of Neptune on February 20, 1874.

The morning was exceedingly foggy, but by noon the Sun broke out and the fog cleared off, but it came on again in the early afternoon, which, in conjunction with light clouds, prevented the disappearance from being observed. The reappearance at the bright limb took place at 6^h 17^m 15^s.34 Greenwich M.T. Observed with the 10-inch refractor and power of 180.

Occultation of ν^2 Cancrī on February 27, 1874.

The star was only occulted by the Moon's limb during eight seconds, but for twenty minutes it ran along the limb, and was frequently occulted for a second or more by lunar elevations.*

	h	m	s
Greenwich M. T. of disappearance	8	33	16.48
„ „ reappearance	8	33	24.48

The clock-error was well-determined on both days; on February 20, from the transit of four stars, and on February 27, from five stars.

* This star was not occulted at the Royal Observatory, Greenwich. It was carefully watched by Mr. Criswick during its progress along the Moon's limb, but on no occasion was it seen to disappear.—[EDITOR.]

Professorship of Astronomy at the Tung-Wén College, Peking.

The Council have been informed that the office of Professor of Astronomy in this College is now vacant. The duty of taking the students who understand the languages of the West through a course of mathematics in the western text-books would devolve upon the Professor of Astronomy. He would also be expected to teach the applications of mathematics in other departments besides that of Astronomy, and to be willing to do the work of a schoolmaster, and yet be able to take charge of an Observatory, if required to do so. He must be acquainted with German and French.

The salary attached to the Professorship will be 600*l.* per annum for the first five years; 800*l.* for the second five; and 1,000*l.* after the tenth year of service. There will be a residence

provided, with some extra perquisites, including allowances for two servants, medical attendance, &c.

Gentlemen desirous of offering themselves as candidates for the vacant Professorship are requested to communicate with Mr. J. D. Campbell, 17 Talbot Road, Bayswater, London, W.

Discovery of Minor Planet (136).

This planet was discovered by M. Palisa, at Pola, on March 18. The following early observations were made at Pola and Berlin:—

			R.A.			N.P.D.		
			h	m	s	h	m	s
Pola	1874	March 18	14	46	39	12	22	2'26
"	"	" 19	9	26	50	12	21	22'80
Berlin	"	" 21	10	29	57	12	19	37'34
								92 50 53'0

Errata.

Vol. xxxiii., page 491, April 21, "Eclipse Reappearance observed by K," for "II. Satellite," read "I. Satellite."

Vol. xxxiv., page 62, "Mr. Burnham's Catalogue of Double Stars," No. 110, fourth column, Decl. 1880, for "—16° 32'," read "—16° 26'."

No. 112, in notes, for "Weisse xii.," read "Weisse xii. 1072."

Page 63, No. 120, third column, R.A. 1880, for "15^h 5^m 1^s," read "16^h 5^m 1^s."